Short-Run Income Shocks and Long-Run Distortions in Household Investments

Minsoo Kim*, Sehoon Kim†, Yoon Kang Lee*, and Hoonsuk Park*

*University of Melbourne
†University of Florida

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Abstract

We show that short-run income shocks can create surprisingly long-run distortions in household investment behavior. Using transaction-level data, we find that households deposit significantly less money into their brokerage accounts for at least two years after a transitory unemployment shock compared to before. This response is stronger for larger shocks and among more constrained households, and driven more by changes in active rather than passive brokerage flows. In particular, deposits remain persistently lower after a household has missed out on higher stock market returns during an unemployment shock. On the other hand, we do not find similar effects for consumption or savings. Overall, our findings are consistent with long-lasting distortions caused by psychological anchors (i.e., "off-ramp effect"), but not fully explained by risk-based explanations.

Keywords: Household Finance, Behavioral Finance, Income Shocks, Unemployment, Brokerage Deposits, Discretionary Investments, Anchoring

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* Minsoo Kim (minsoo.kim@unimelb.edu.au), Yoon Kang Lee (yoonkang.lee@unimelb.edu.au), and Hoonsuk Park (hoonsuk.park@unimelb.edu.au) are with the University of Melbourne, Faculty of Business and Economics, Department of Finance.

† Sehoon Kim (sehoon.kim@warrington.ufl.edu) is with the University of Florida, Warrington College of Business, Department of Finance, Insurance, and Real Estate.

"Never be out and think you can get back in because your emotions will defeat you totally"

– Jack Bogle, Founder of Vanguard

1 Introduction

Sound financial decisions are the foundations of household welfare (see Campbell, 2006; Calvet, Campbell, and Sodini, 2007, 2009; Lusardi and Mitchell, 2007). As a determinant of household portfolio choice, labor income is often thought to affect decisions through shifts in permanent income, liquidity constraints, or risk parameters (see Hall, 1978; Deaton, 1991; Guiso, Jappelli, and Terlizzese, 1996). However, cognitive limitations and behavioral biases are widely considered to impact the ability of households to make optimal financial decisions (see Vissing-Jørgensen, 2003; Grinblatt, Keloharju, and Linnainmaa, 2011). In this paper, we provide novel evidence that temporary income shocks can impact household investment behavior by creating reference points that trigger behavioral biases (see Heath, Huddart, and Lang, 1999; George and Hwang, 2004).

We analyze proprietary transaction-level data covering a large sample of U.S. households. In difference-in-differences (DID) estimates, we find that brief periods of unemployment, identified using unemployment insurance (UI) receipts, are followed by persistent and significant reductions in household financial investments indicated by brokerage account deposits. In economic magnitudes, the results imply a one percentage point greater decline in brokerage deposits as a fraction of pre-sample income for treated households compared to matched households. Given a historical savings rate of 9% among U.S. households, this is a significant reduction.[1] More importantly, while the average income shock lasts for four to five months, the decline in brokerage deposits lasts at least two years or more after the shock has completely dissipated and household income has recovered to previous levels.

This persistence cannot be explained by mere lack of funds during brief declines in income under neoclassical models of household behavior. While financing and liquidity constraints

[1] See FRED economic data from the St. Louis Federal Reserve Bank.

can lead households to temporarily curtail their brokerage investments in response to transitory income shocks (see Johnson, Parker, and Souleles, 2006; Agarwal and Qian, 2014; Baker, 2018), the long-lasting effects that follow are more consistent with potential mechanisms related to behavioral biases or risk aversion. We examine these channels in additional tests.

Our primary hypothesis is based on theories of non-standard investor preferences, where agents derive utility from gains and losses relative to reference points (see Tversky and Kahneman, 1974; Barberis and Xiong, 2012).[2] We posit that transitory income shocks are effectively one-way *"off-ramps"* for household investments. Under this hypothesis, a constrained household that cuts back on its investments during a shock finds it psychologically difficult to ramp its investments back up to pre-shock levels, even after the household's income returns to normal. For instance, asset prices may have risen while the household was "missing out" because of the shock, biasing the household to anchor on involuntarily forgone investment returns. As a result, loss-averse households become reluctant to *realize forgone returns* by buying assets that have risen in value, persistently investing less than before.

We provide several additional evidence supporting this channel. First, our subsample analysis indicates that households' constraints affect their responses during temporary income shocks. Specifically, our main results are more pronounced for households with lower ex-ante income levels prior to income shocks and for larger temporary income losses. While constraints help explain why households respond to brief declines in income *during* the shocks, they alone do not explain why this response persists *long after* the shocks.

Next, we provide evidence to help explain this persistence from the lens of household behavioral biases. We start by examining households' activeness (as opposed to passiveness) in their brokerage deposits as an indication of their susceptibility to such biases. Leveraging details on each household's deposit transactions in our data, we document greater and more persistent declines in the discretionary component of an affected household's brokerage de-

[2] The effects of reference points and psychological anchors are widely documented in financial markets (see Heath et al., 1999; George and Hwang, 2004; Li and Yu, 2012), housing markets (see Anderson, Badarinza, Liu, Marx, and Ramadorai, 2020), and experimental settings (see Frydman, Barberis, Camerer, Bossaerts, and Rangel, 2014).

posits than its passive component. We also find that households who were more active in making non-passive discretionary deposits into their brokerage accounts prior to temporary shocks are more likely to persistently invest less afterwards.

To further attribute our findings to psychological off-ramps, we examine an anchoring hypothesis where temporary income shocks introduce reference points that interfere with household investment decisions. Specifically, we test whether households who have involuntarily forgone investment returns during income shocks are less likely to increase discretionary investments back to pre-shock levels afterwards. Consistent with this hypothesis, we find that household brokerage deposits persistently remain lowered, particularly when the S&P500 index had performed *well* since the onset of the shock faced by the household. Furthermore, anchoring is evident primarily in the discretionary (i.e., active) component of the household's brokerage deposits, but not in the passive component of deposits.

An alternative explanation could be that the brief but salient experience of unemployment increases the risk aversion of affected households (see Malmendier and Nagel, 2011), reducing their willingness to invest in risky assets. Another related explanation might be that heightened background income risk reduces household investments (see Bodie, Merton, and Samuelson, 1992; Pratt and Zeckhauser, 1987; Kimball, 1993; Gollier and Pratt, 1996; Guiso et al., 1996). While these explanations are partially consistent with our results, they cannot fully explain our suite of findings. In particular, we do not find that the temporary income shocks persistently affect households' propensity to consume in the same way they affect their brokerage deposits. This is inconsistent with a pure risk-appetite or background risk story, as households' consumption and saving behavior should be impacted by changes in their general tolerance for risk or the riskiness of their income streams.

Another alternative explanation may be that households have limited attention, and are therefore unlikely to naturally revert their behavior once altered by a large income shock. However, this "menu hypothesis" does not hold much ground through our results either. There is no evidence of a persistent decline in the *passive* component of brokerage deposits, which is where a lack of attention would most likely manifest as stickiness. Moreover, the *active* component of deposits declines sharply both during and long after the shock, especially if households have forgone higher returns (i.e., stock markets had risen substantially during the shock). These are inconsistent with an attention-based menu hypothesis.

Our study contributes to the rich household finance literature (see Campbell, 2006; Gomes, Haliassos, and Ramadorai, 2021). Central to this literature are investigations of household investment behavior that seem like discrepancies from standard models.[3] These include portfolio under-diversification, stock market non-participation, and selling winners while keeping losers, among others (see Grinblatt and Keloharju, 2001; Calvet et al., 2007, 2009; Wachter and Yogo, 2010). Several factors have been shown to contribute to these household finance puzzles, such as education and financial literacy (see Van Rooij, Lusardi, and Alessie, 2011; Behrman, Mitchell, Soo, and Bravo, 2012; Von Gaudecker, 2015), social interactions and networks (see Hong, Kubik, and Stein, 2004; Brown, Ivkovic, Smith, and Weisbenner, 2008), liquidity or credit constraints (see Becker and Shabani, 2010; Briggs, Cesarini, Lindqvist, and Östling, 2021), and other demographic factors such as age, intelligence, or health (see Grinblatt et al., 2011; Rosen and Wu, 2004). Another strand of this literature examines behavioral biases, preferences, or beliefs to help understand many of the puzzles in household portfolio choices (see Polkovnichenko, 2005; Dimmock, Kouwenberg, Mitchell, and Peijnenburg, 2016, 2021; Malmendier and Nagel, 2011; Beshears, Choi, Laibson, and Madrian, 2018). Our findings contribute to this literature by showing that short-term perturbations in the labor income process can influence household portfolio choice in ways that are not well-explained by standard models of income and investment, but better explained by behavioral and psychological mechanisms.

Finally, our study also contributes to the literature on household income and savings (see Caballero, 1990; Carroll, Hall, and Zeldes, 1992). Many studies document that household consumption is sensitive to transitory movements in income (see Hall and Mishkin, 1982).

[3] Campbell (2006) calls these *investment mistakes*.

This has often been explained by liquidity and credit constraints that sit well with neoclassical models of household behavior (see Carroll, 2001; Gross and Souleles, 2002; Blundell, Pistaferri, and Preston, 2008; Parker, Souleles, Johnson, and McClelland, 2013). However, these sensitivities have also been shown to be particularly large for downward income shocks (see Shea, 1995; Bunn, Le Roux, Reinold, and Surico, 2018) or movements in income relative to peers (see Attanasio and Davis, 1996), suggesting that non-standard preferences (e.g., loss aversion) can help explain important aspects of household behavior. While much of this literature focuses on household consumption behavior, our findings shed new light on the interplay between transitory income shocks and households' financial investments.

2 Empirical Strategy

2.1 Data

The data used in our analyses are obtained from an online account aggregator that allows households to pool their financial information from various bank and credit card accounts in one platform (see Baugh, Ben-David, and Park, 2018; Baugh, Ben-David, Park, and Parker, 2021). This service enables subscribed households to conveniently view and manage their financial activities, for example, by monitoring their spending and investments or by signing up for alerts for upcoming bills and approaching credit limits. Households subscribe to the service and provide their login credentials associated with various financial accounts, so that the service can automatically access and extract information on bank and credit card transactions. Once a household joins the service, the aggregator has access to this information until the household actively discontinues service by requesting account deletion. Therefore, there is low attrition in the data after households subscribe.

The raw data consist of daily transactions for 2.7 million households from July 2010 to May 2015, covering all banking (e.g., checking, savings, and debit card) and credit card transactions for any account linked to the service by each household. In a format similar to what is typically found on bank or credit card statements, we observe the date, amount, and description of each transaction. In addition, the aggregator also classifies each transaction into several categories, which facilitates analyses of different types of transactions. Finally, each household is assigned a unique and permanent identifier, allowing us to track each household's behavior over time.

2.2 Empirical Setting

From this data, we retrieve two critical pieces of information: temporary income shocks and household financial investments. First, we identify temporary income shocks based on the receipt of unemployment insurance (UI) benefits by households. UI benefits are useful in this context because most U.S. states require that UI applicants prove that they are unemployed and actively seeking employment. Setting aside UI applications, which are not publicly observable at the household level, UI receipts observed in granular household-level transaction data provide a clean way of identifying income shocks arising from unemployment.

To account for the lag between UI applications and receipts (e.g., three-to-six weeks in New York), we identify periods of income shocks as starting two months before the household's initial UI receipt and lasting until its final UI receipt. For households that continue to exhibit zero salary income after exhausting UI benefits (see Ganong and Noel, 2019), we define the end of their income shock periods as when they resume positive salary income.

To ensure that income shocks are temporary, we require that the household's average salary income in the six months after the end of the income shock period does not differ by more than 25% from their average salary income during the three months prior to the shock. In other words, we define a temporary income shock as a cleanly identified unemployment shock followed by a nearly complete restoration of income. We define households that experience such temporary shocks during our sample period as "treated households".

Second, we measure households' financial investments based on deposits into (and withdrawals from) their brokerage accounts, observed in our data as transactions between bank and brokerage accounts. For example, a bank debit transaction described as "ACH Electronic Debit SCHWAB BROKERAGE MONEYLINK - AMOUNT 12000" indicates a $12,000 deposit into a Charles Schwab brokerage account. Based on the transaction codes and descriptions, we capture household-level investment flows to the top brokerage firms in the U.S.[4] More details about the data and variable construction can be found in the Appendix.

2.3 Sample Overview

To examine how temporary income shocks affect household investment behavior, we construct a matched panel data set consisting of treated households that experience such income shocks and observably similar control households that do not. This enables us to study the differential change in financial investments by households before and after temporary income shocks, in comparison to a counterfactual group of households. To qualify as a matching candidate, a control household must never experience any income shock during our sample period as indicated by UI receipts, temporary or otherwise.

We match each treated household with a control household that has the nearest propensity score estimated in a logistic regression using total consumption, income, and brokerage deposits averaged over the three-month period preceding the onset of the income shock. We also match on whether the household had been a *passive* or *active* depositor during the year prior to the shock. Specifically, households are defined as passive if they made at least 75% of their brokerage deposits during this period in regular intervals and recurring amounts, and are otherwise classified as active.[5] Each pair of a treated and matched household is then tracked over the same period from three months before the income shock to at least seven months after the shock.

[4]These brokerage firms include Charles Schwab, TD Ameritrade, E*TRADE, Fidelity, Franklin Templeton Investments, Merrill Lynch Funds, Oppenheimer & Co., Morgan Stanley Smith Barney, ING Direct Investments, Interactive Brokers, Invesco Investments, Raymond James, Scottrade, Vanguard, AllianceBernstein Holding, American Century Investments, American Funds Investments, Edward Jones Investments, Janus Capital Group, Pershing LLC, Putnam Investments, Sharebuilder Securities Corp, State Farm Investment Management Corp, USAA Mutual Funds, Utah Educational Savings Plan, and Waddell & Reed Financial.

[5]We define "recurring amounts" as up to five transaction values that recur in fixed patterns.

Table 1 reports summary statistics for this matched sample, which consists of 5,504 households. On average, a temporary income shock lasts for 21 weeks (median 15 weeks) and constitutes a decline in monthly household salary income of 35% relative to the pre-shock three-month average. Further corroborating the transitory nature of these shocks, Figure 1 (Panel A) shows that the temporary drop in income for the average treated household fully recovers within three months after the end of the shock period. On average, households in our sample earn $8,403 in salary income, consume $2,737, and deposit $403 in their brokerage accounts each month, indicating that these are middle-class households.

[Insert Table 1 here]

[Insert Figure 1 here]

Figure 2 illustrates the closeness of observable covariates between treated and matched households, ensuring that the matches are of high-quality. Across all variables used in our matching procedure, there are no statistically significant differences between treated and matched households prior to the temporary income shocks.

[Insert Figure 2 here]

3 Results

3.1 Main Results

Based on this matched sample, we estimate the following household-week panel regression to study the effects of temporary income shocks on household investment behavior.

$$
\begin{aligned}
Y_{i,t} = {} & \gamma \cdot Treat_i \times Shock_{i,t} + \delta_1 \cdot Treat_i \times PostShockST_{i,t} + \delta_2 \cdot Treat_i \times PostShockLT_{i,t} \\
& + \theta \cdot Shock_{i,t} + \kappa_1 \cdot PostShockST_{i,t} + \kappa_2 \cdot PostShockLT_{i,t} + \lambda' \cdot X_{i,t-1} + \mu_i + \nu_t + \epsilon_{i,t}
\end{aligned}
\tag{1}
$$

$Y_{i,t}$ denotes one of the following outcomes variables for household i in week t: brokerage deposits (baseline dependent variable), net brokerage flows (i.e., deposits net of withdrawals), or brokerage withdrawals, all scaled by the household's average weekly salary income during the three months prior to entering our sample. $Treat_i$ denotes a dummy variable indicating whether household i experiences a temporary income shock during our sample period.

To assess the persistence of the impact of temporary income shocks, we assign dummy variables for three different sub-periods around a shock experienced by a household (or pseudo-shock for a matched household). Covering the duration of the temporary income shock itself, $Shock_{i,t}$ indicates whether household i is undergoing an income shock in week t. Next, for the period shortly after the shock has dissipated, $PostShockST_{i,t}$ indicates weekly observations within the first six months after household i has exited an income shock period. Finally, capturing long-term effects, $PostShockLT_{i,t}$ indicates household-week observations *at least* seven months after the end of an income shock (further decomposed into <1yr or >1yr in some specifications). The omitted base period is the three-month pre-shock period.

The key coefficients of interest are on the interaction terms, γ, δ_1, and δ_2, which capture the differential impact of temporary income shocks on household financial investments during, shortly after, and long after the shocks. μ_i and ν_t denote household and week fixed effects, respectively. In specifications where we control for household fixed effects, we also control for matched pair fixed effects (i.e., treated and matched household pairs). Standard errors are adjusted for clustering at the household level.

The results are reported in Table 2. In the first six columns, we examine household brokerage deposits as our baseline outcome variable. Across these specifications, we gradually add more granular household, matched pair, and week fixed effects to the regressions. In all specifications, we find consistent and robust evidence that treated households significantly reduce deposits into their brokerage accounts compared to matched households after temporary income shocks. This reduction occurs not only during or shortly after the temporary shocks, but also long after these shocks are completely resolved such that households' salary income will have fully recovered to previous levels.

[Insert Table 2 here]

In economic magnitudes, the coefficient on the interaction term, $Treat_i \times Shock_{i,t}$, indicates an approximately one percentage point greater decline in brokerage deposits as a fraction of pre-sample income for treated households compared to matched households (see column 5 for the most stringent fixed effects specification). This corresponds to a 20% relative decrease in deposits with respect to the pre-shock sample mean.

More importantly, the magnitude and statistical significance of this decline persists even after the income shocks have passed. The coefficients on the interaction terms, $Treat_i \times PostShockST_{i,t}$ and $Treat_i \times PostShockLT_{i,t}$, are both large, indicating a persistent relative decline in brokerage deposits amounting to 0.8–0.9% of pre-sample income. Column 6 of Table 2 further breaks down the $PostShockLT_{i,t}$ term into two long-term post-shock sub-period dummy variables, one indicating seven months to a year after the end of the income shock period (<1yr) and another indicating beyond one year after the shock period (>1yr). This specification shows that the fall in brokerage deposits persists and maintains its magnitude even beyond a full year after a temporary income shock has dissipated.

Columns 7 and 8 in Table 2 show that these results are robust to replacing brokerage deposits with net brokerage flows as the outcome variable. Columns 9 and 10 further show that these responses are primarily driven by deposits rather than withdrawals: there is no evidence that households change the amount of withdrawals from their brokerage accounts.

3.2 Difference-in-Differences Dynamics

To validate our baseline difference-in-differences (DID) strategy above, we visually inspect the dynamics of household brokerage flows before and after temporary income shocks. Specifically, we report and plot coefficients from the following household-week panel regression, estimated separately for treated and matched households.

$$
\begin{aligned}
Deposits_{i,t} = & \sum_{m=-3}^{-1} \eta_m \cdot PreShock[m]_{i,t} + \theta \cdot Shock_{i,t} + \sum_{m=+1}^{+7\sim} \kappa_m \cdot PostShock[m]_{i,t} \\
& + \lambda' \cdot X_{i,t-1} + \mu_i + \nu_t + \epsilon_{i,t}
\end{aligned} \tag{2}
$$

The indicator variable for the temporary income shock period, $Shock_{i,t}$, is defined as in the previous baseline analysis. Instead of omitting the pre-shock period and including two post-shock sub-period dummy variables, here we include individual time dummies for each month of the pre-shock and post-shock periods. These time dummies are denoted as $PreShock[m]_{i,t}$ and $PostShock[m]_{i,t}$, covering the period from three months before the shock (i.e., omitted category) to seven months or more after the passing of the shock (i.e., last category).

Alternatively, we estimate the following augmented regression on the pooled sample of treated and matched households, where we interact the individual time dummies with the household treatment variable, $Treat_i$.

$$
\begin{aligned}
Deposits_{i,t} = & \sum_{m=-3}^{-1} \beta_m \cdot Treat_i \times PreShock[m]_{i,t} + \gamma \cdot Treat_i \times Shock_{i,t} \\
& + \sum_{m=+1}^{+7\sim} \delta_m \cdot Treat_i \times PostShock[m]_{i,t} + \sum_{m=-3}^{-1} \eta_m \cdot PreShock[m]_{i,t} + \theta \cdot Shock_{i,t} \\
& + \sum_{m=+1}^{+7\sim} \kappa_m \cdot PostShock[m]_{i,t} + \lambda' \cdot X_{i,t-1} + \mu_i + \nu_t + \epsilon_{i,t}
\end{aligned} \tag{3}
$$

The first two columns in Table 3 – also visualized in Figure 1 (Panels B and C) – report results from estimating equation 2 separately for treated and matched households. These results clearly illustrate parallel trends in brokerage deposits by treated and matched households prior to temporary income shocks, but substantial divergence afterwards. The $PreShock[m]_{i,t}$ terms are generally insignificant in both groups, whereas the $PostShock[m]_{i,t}$ terms are negative, large, and statistically significant only for treated households. The magnitudes of the $PostShock[m]_{i,t}$ terms are comparable to those of our baseline DID estimates.

The third column also demonstrates this result in pooled regressions estimating equation 3.

[Insert Table 3 here]

In particular, the post-shock divergence is clearly driven by treated households decreasing their deposits even long after the passing of temporary income shocks. While Panel A of Table 3 shows the persistence of this divergence up to seven months or more after the passing of income shocks, Panel B extends the analysis by disaggregating the last time category ($+7m\sim$) into post-shock monthly dummies of up to 14 months or more. The results show strongly robust evidence of persistent declines in brokerage deposits by treated households, lasting well beyond a full year after the complete recovery of income.

3.3 Economic Channels

Thus far, we have documented that temporary income shocks can have strikingly persistent effects on the investment behavior of households. This is puzzling from a neoclassical perspective. While financing or liquidity constraints may temporarily push households to curtail investments in the face of an income shock, these constraints should not bind after the shock has passed and household income has recovered. Therefore, constraints alone cannot explain why short-lived shocks should have long-lived consequences. We consider several mechanisms that might explain our baseline results.

Under our primary hypothesis, temporary income shocks force constrained households to curtail investments, causing them to involuntarily forgo investment returns if asset prices rise during the shock. In turn, loss-averse households anchor on the forgone returns they have "missed out" on, finding it psychologically difficult to *realize forgone returns* by buying assets that have risen in value. As a result, households fail to ramp up their investments back to previous levels even after the shock has passed, persistently investing less than before. In other words, the temporary income shocks are effectively one-directional *"off-ramps"*.

We also consider several alternative channels. For instance, a temporary shock might serve as a salient experience that reduces the risk-tolerance of impacted households (see Malmendier and Nagel, 2011), or might increase background income risks for households (see Bodie et al., 1992; Pratt and Zeckhauser, 1987; Kimball, 1993; Gollier and Pratt, 1996; Guiso et al., 1996). These risk channels may affect households' appetite for risky investments. Alternatively, households might alter their investment behavior in the face of large shocks, but might not be attentive enough to revert their investments afterwards. We conduct several tests to investigate these potential economic channels.

3.3.1 Household Constraints

First, we examine the role of households' constraints in their *immediate* responses to temporary income shocks. Setting aside the *persistence* of this response, standard models predict that liquidity or financing constraints faced by households can shape how temporary income shocks impact their behavior (see Carroll, 2001). As such, household constraints are important sources of variation to understand how brokerage deposits change *during* these shocks.

As a proxy for household constraints, we first consider household income levels prior to the temporary income shocks. Intuitively, households that had earned more income ex ante would have been able to preserve more liquidity that can serve as a buffer in the face of an income shock. Specifically, we partition our sample of households into terciles based on their average income during the three months prior to the temporary income shocks. We sort the treated households while maintaining their matched counterparts.

Alternatively, we partition the sample by the magnitude of income loss stemming from the temporary shocks. Intuitively, a household that suffers a larger percentage drop in its income due to an unemployment shock (e.g., the primary wage-earner in the household loses her job) would be more constrained during the shock compared to a household that only suffers a moderate decline in income (e.g., one household member loses her job but another member remains the primary earner). Similarly, we partition our sample of households into terciles based on the percentage decline in income (i.e., post-shock average income plus UI benefits, relative to pre-shock average income).

The results from estimating our baseline regression on these subsamples are presented in Table 4. The first two columns show results for households in the bottom and top terciles in terms of pre-shock household income. Clearly, households with low ex ante income respond to temporary income shocks by reducing their brokerage deposits much more sharply and significantly than households with high ex ante income. The difference between low and high income households' responses is substantial in magnitude (e.g., coefficients of -1.26 vs. -0.57 on the $Treat \times Shock$ term), and statistically significant at 5%. The next two columns contrast results between households that experience a large or small percentage decline in income during the temporary shock. Similarly, households that experience a large shock respond much more prominently than households that go through a smaller shock. The difference between the two groups is equally striking (e.g., coefficients of -2.06 vs. -0.74 on the $Treat \times Shock$ term). These results indicate that temporary income shocks disproportionately impact the investment behavior of constrained households.

[Insert Table 4 here]

However, the fact that constrained households respond more sharply *during* temporary income shocks than unconstrained households does not necessarily mean that constraints fully explain how *persistent* those responses are after income shocks have passed. If they did, the coefficients that capture the degree of persistence – those on the interaction terms $Treat \times PostShockST$ and $Treat \times PostShockLT$ – should only be statistically significant and economically meaningful for constrained firms.

This is not true in our data. Table 4 shows that households with high ex ante income persistently reduce their brokerage deposits in the six months *after* income shocks have passed (i.e., coefficient on $Treat \times PostShockST$ term of -0.70, significant at 5%), at least as much as they had reduced deposits *during* the shocks (i.e., coefficient on $Treat \times Shock$ of -0.57, significant at 10%). Similarly, households who experienced small shocks deposit significantly less into their brokerage accounts long after the shocks have dissipated (i.e., coefficients on

$Treat \times PostShockST$ and $Treat \times PostShockLT$ of -0.59 and -0.54, significant at 5% and 10%, respectively). Moreover, relative to the response *during* the shock as the base, these *post-shock* responses are at least as sizeable for households that underwent small shocks compared to households that experienced large shocks.

In short, constraints help explain how households adjust their financial investments during temporary income shocks, but not why they continue to respond persistently afterwards.

3.3.2 Active vs. Passive Investments

Next, we explore channels related to behavioral biases. Before we focus on a specific bias, we start with the general premise that active investors are more likely to be subject to various biases. It is widely documented in the literature, for instance, that excessive trading is often motivated by psychological factors such as loss aversion, trend extrapolation, anchoring, and overconfidence (see, e.g., Odean, 1999; Grinblatt and Keloharju, 2001). We therefore examine whether households alter their investment behavior in response to temporary income shocks more persistently if they are more active as investors.

To operationalize this empirically, we leverage our granular household transaction level data. First, we classify brokerage deposit transactions within households as *passive* or *active* deposits. Specifically, brokerage deposits are defined as *passive* if they occur in fixed intervals (i.e., weekly, biweekly, monthly, bimonthly, or quarterly) and in identical amounts for at least six transaction occurrences. Deposit transactions that do not satisfy these conditions are otherwise viewed as discretionary deposits, and thus classified as *active*. We then construct two deposit variables for each household-week observation: one for the active component and another for the passive component of the household's brokerage deposits. Alternatively, we take a coarser approach and split the cross section of households into active and passive households based on our definition in Section 2.3.

Based on these approaches, we analyze the heterogeneity of our results across active and passive household investments. Columns 1 and 2 of Table 5 report results from running two regressions with different outcome variables – active vs. passive deposits – on the full sample. The results show that households reduce the active component of their brokerage deposits not only during temporary income shocks but even long afterwards, whereas they reduce passive deposits only during the shock but not afterwards. The coefficients on the terms interacting $Treat$ with the $Shock$, $PostShockST$, and $PostShockLT$ are all economically large and statistically significant for active deposits (i.e., -0.63, -0.76, and -0.64, all significant at 1%). In sharp contrast, only the $Treat \times Shock$ term is significant for passive deposits (i.e., -0.15, significant at 5%). Furthermore, the differences in the coefficients between the two specifications are always highly significant. This contrast also holds in the cross-section of active and passive households. In columns 3 and 4, we show that the effects of temporary income shocks are almost twice as strong for active households than for passive households.

[Insert Table 5 here]

These findings clearly demonstrate that households alter their discretionary investments more significantly and persistently in response to temporary shocks, than the investments they set up to occur passively. However, it is worth noting that in our approach, active or passive investing does not refer to the types of investments households make or how often they buy and sell securities. Given that we are constrained in our data to transactions at the household account level, we are unable to make stronger claims that require household-security-level transaction data. We caveat this limitation in interpreting our findings.

3.3.3 Anchoring on Forgone Investment Returns

Building on this behavioral premise, we further test an anchoring hypothesis based on theories of non-standard preferences where agents derive utility from gains and losses relative to reference points (see Tversky and Kahneman, 1974; Barberis and Xiong, 2012). Lending motivation for this approach, it is widely documented that psychological anchors on reference points affect asset market outcomes in important ways (see Heath et al., 1999; George and Hwang, 2004; Li and Yu, 2012; Frydman et al., 2014; Anderson et al., 2020).

Under this hypothesis, temporary income shocks force constrained households to involuntarily forgo investment returns over the period of the shock. Subsequently, households psychologically anchor on these forgone returns as reference points. As a result, households remain reluctant to increase their discretionary investments back to pre-shock levels even after their income levels recover, because they are averse to *realizing* the forgone returns they have missed out on. We refer to this as the "off-ramp" effect.

A prediction unique to this hypothesis is that the persistence of households' responses to temporary income shocks are positively correlated with the magnitude of forgone returns. To empirically test this prediction, we employ the cumulative return on the S&P500 index during a household's temporary income shock period as a proxy for the household's forgone return.[6] We then examine whether our main findings of persistently lowered brokerage deposits are magnified for treated households that have forgone higher investment returns.

The results are reported in Table 6. We estimate our baseline regression in equation 1 for two subsamples: One including treated households in the *top tercile* of forgone returns and their matched households (i.e., "High" forgone returns), and another containing treated households in the *bottom tercile* of forgone returns and their matched counterparts (i.e., "Low" forgone returns). We report results for each of these subsamples based on the active and passive components of household brokerage deposits as alternative dependent variables.

[Insert Table 6 here]

Consistent with an off-ramp effect, we find that households decrease their brokerage deposits – and persistently keep them lowered – following temporary income shocks, primarily when the S&P500 index had performed well since the onset of the shocks they had faced. Furthermore, this conditional effect is strikingly clear in the discretionary (i.e., active) component of the household's brokerage deposits, but not in passive deposits.

[6] We use the return on the S&P500 index as a proxy because we do not directly observe household portfolio holdings in our data. However, recent studies show that the average household portfolio beta is close to one (see, e.g., Gabaix, Koijen, Mainardi, Oh, and Yogo, 2023), indicating that the market return is an imperfect but a reliable proxy for unconditional household portfolio returns.

The first two columns show the results for active deposits. In the "high" forgone return subsample, the coefficients for all three interaction terms, $Treat \times Shock$, $Treat \times PostShockST$, and $Treat \times PostShockLT$, are significant both economically and statistically (see column 1). Importantly, the magnitude of decline in deposits during both of the post-shock periods (i.e., -1.08 and -0.79) are at least as large as that during the shock period (i.e., -0.80), highlighting the persistence of the decline. In sharp contrast, we do not observe such persistence in the "low" forgone return subsample (see column 2). The decline in deposits during the post-shock periods (i.e., -0.39 and -0.38) are neither large nor significant compared to the decline during the shock period (i.e., -0.75).

It is noteworthy that the degree of *persistence* of the decline in deposits *after* the shocks have passed are markedly different between the two subsamples, whereas the decline *during* the shocks are not. In particular, the p-value from comparing the coefficients on $Treat \times Shock$ between the high and low subsamples is 0.45, indicating that household responses *during* the shocks do not depend on the returns households are missing out on. This supports our conjecture that households' initial responses to temporary income shocks need not be a behavioral phenomenon, but the inability of households to revert their responses after the shocks have passed is likely driven by cognitive factors. Further corroborating this behavioral interpretation, the last two columns in Table 6 show that, by and large, there is no persistent decline in passive deposits, nor any difference in this decline conditional on the amount of forgone returns.

3.3.4 Alternative Explanations

Risk-Tolerance and Income Risk Profiles

Next, we consider other channels that might explain our baseline results. One alternative explanation is that the brief but salient experience of unemployment might induce affected households to become more risk-averse (see Malmendier and Nagel, 2011). This may reduce the appetite of households for risky assets. Another explanation is that temporary income shocks might be manifestations of heightened background income risk, which may also reduce investments by risk-averse households (see Bodie et al., 1992; Pratt and Zeckhauser, 1987; Kimball, 1993; Gollier and Pratt, 1996; Guiso et al., 1996).

Leveraging our rich transaction-level data, we assess these possibilities by inspecting household behavior more generally. For instance, changes in income risk profiles or households' perceptions about such risks should be associated not only with changes in household financial investments but also with changes in consumption and savings behavior. From our data, we construct household-week consumption by aggregating transactions with keywords associated with retailers, such as big box stores, specialty stores, groceries, and restaurants. We also include as consumption all credit card transactions that are not credit card payments, interest, or other charges. As a proxy for savings, we also analyze interest income, which is constructed from transactions that are inflows into bank accounts and contain the keyword "interest". We utilize this information to test whether the aforementioned risk-related channels are the primary drivers of our results.

The results are reported in Table 7. In Panel A, we use household consumption and interest income, both scaled by the average income of the household over the three-month period prior to entering our sample, as outcome variables instead of brokerage deposits. We do not find that temporary income shocks persistently affect either of these variables. This is inconsistent with a pure risk-based explanation, as households' propensity to consume or save should be affected by changes in their appetite for risk or the riskiness of their income streams.

[Insert Table 7 here]

In Panel B, we conduct our baseline analysis of household brokerage deposits on tercile subsamples sorted on the magnitude of percentage decline in household consumption during the temporary income shock period. We continue to find robust evidence of persistent and significant declines in brokerage deposits in the top and bottom tercile samples, and that there are no significant differences between the extreme terciles in the estimated coefficients. In other words, changes in household consumption behavior during the shock are unrelated to the strength and persistence of changes in household financial investments. Overall, risk-based explanations cannot fully account for the suite of our findings.

Limited Attention and Menu Hypotheses

Another alternative explanation is that households exhibit limited attention, and are therefore slow to naturally correct their behavior once altered by a large shock. However, this "menu hypothesis" predicts that the persistent effects of temporary income shocks are pronounced in the *passive* component of brokerage deposits that households pay less attention to under normal circumstances. Contradicting this prediction, however, we find that the *active* component of deposits declines much more sharply and persistently after the shock, especially when households have missed out on investment returns during the shock. This is inconsistent with an attention-based menu hypothesis.

4 Conclusion

In this paper, we document that temporary income shocks can have long-lasting effects on household financial investments, even though household income levels return to normal after the passing of these shocks. Specifically, we leverage a proprietary transaction-level dataset covering a large sample of U.S. households to show that households significantly reduce discretionary deposits into their brokerage accounts after brief unemployment shocks. These effects last for up to at least two years after the full resolution of the shocks, which is puzzling from the perspective of standard models of household behavior.

We provide several pieces of evidence that are consistent with an "off-ramp" effect, whereby temporary shocks force constrained households to forgo investment returns for a brief period, which then makes it psychologically difficult for households to return to their pre-shock investment patterns due to anchoring on those forgone returns. Specifically, we find that the initial response to the temporary shocks are stronger for ex ante constrained households, but that the persistence of this response is driven by active deposits and high forgone returns. On the other hand, we do not find evidence supporting risk-based mechanisms or attention-based passive inertia as alternative explanations.

Our study has important implications for household finance. Jack Bogle, founder of Vanguard Group, famously said that investors should not exit markets for emotions would prevent them from re-entering. We find that external factors from the real economy, such as temporary income shocks, can force households to make this very mistake. We also show evidence consistent with the role of cognitive biases in preventing households from correcting the changes they make to their investment policies even after these shocks have dissipated. In short, our findings highlight that transitory labor income fluctuations can psychologically distort long-term household financial decisions.

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Figure 1. Household Income and Brokerage Flow Dynamics

These figures plot the dynamics of household level income and brokerage deposits before and after temporary income shocks (or pseudo-shocks for matched households). We estimate equation 2 separately for treated and matched households, by running regressions of household income (Panel A) or brokerage deposits (Panels B and C) on an indicator variable for the temporary income shock period (*Shock*) and individual time dummies for each month of the pre-shock (*PreShock*[m]) and post-shock (*PostShock*[m]) periods, covering from three months before (i.e., omitted category) to up to 14 months or more after the passing of the shock. The coefficients on the time dummies and their 95% confidence intervals are plotted. Panels A and B show household income and brokerage flow dynamics for up to seven months or more after the passing of shocks, respectively. Panel C further disaggregates the *PostShock*[$7m+$] dummy into monthly dummies for up to 14 months or more for brokerage deposits. Household and week fixed effects are included in all specifications. Standard errors are adjusted for clustering at the household level (*** p<0.01, ** p<0.05, * p<0.1).

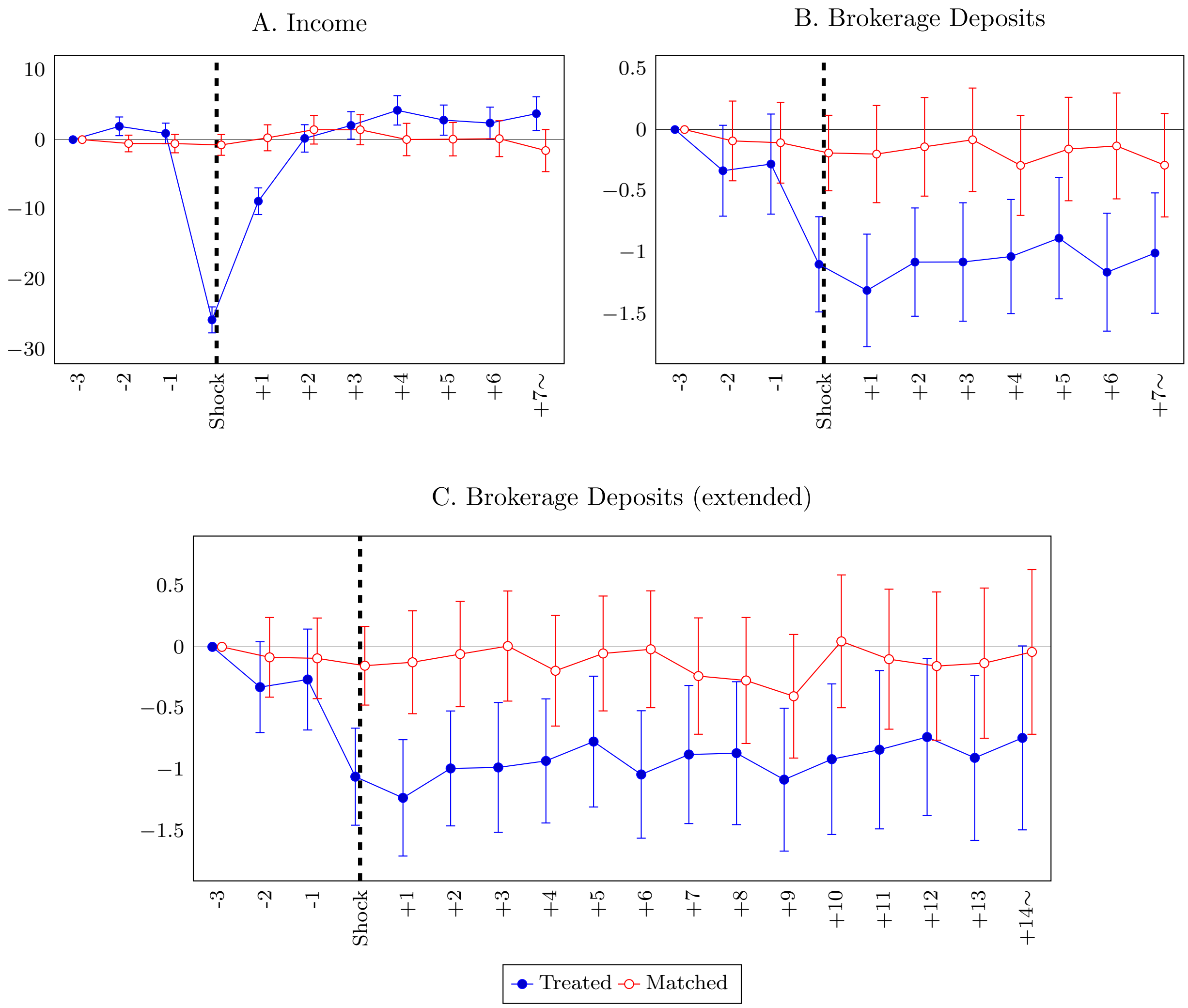

Figure 2. Covariate Balancing of Treated and Matched Households

This figure plots the balancing of covariates between treated and matched households. The standardized mean difference between treated and non-treated households along with its 95% confidence interval is plotted for each covariate averaged over the pre-shock sample period, both before and after matching. The standardized mean difference is the coefficient from regressing each covariate on a treatment dummmy, controlling for dummies indicating shock start-weeks. The confidence intervals are computed based on robust standard errors of the coefficients.

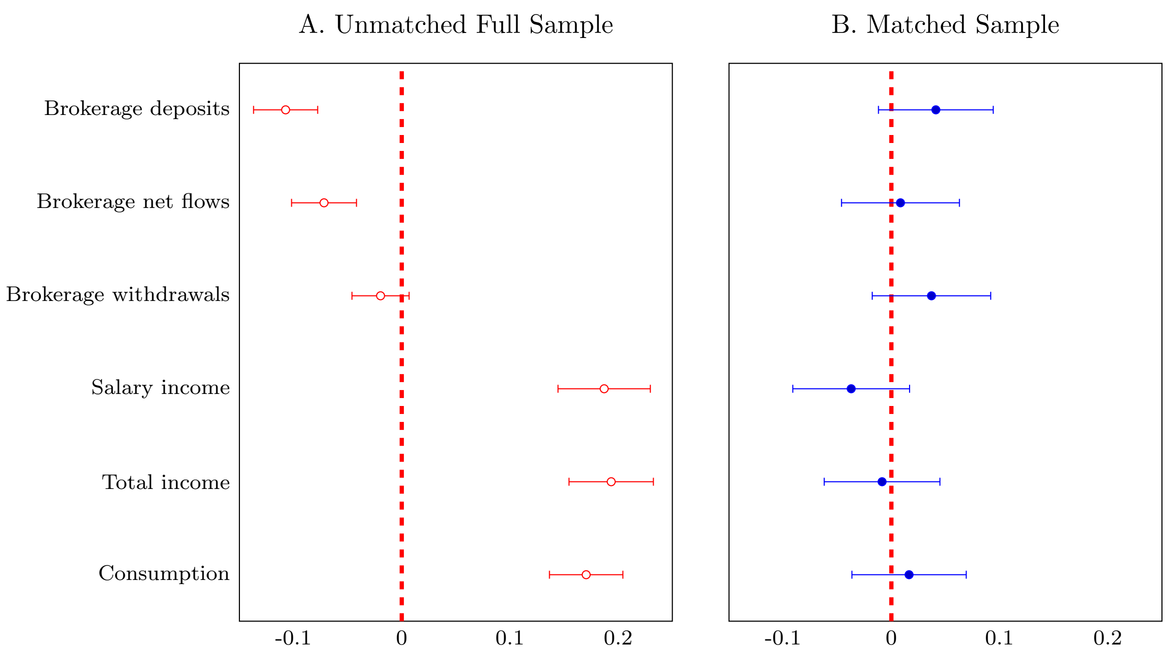

Table 1. Summary Statistics

This table presents summary statistics of key variables aggregated at the household-month level over the sample period from August, 2010 to May, 2015. *Unemployment shocks* are the percentage decline in average monthly income during temporary income shocks for treated households, relative to their pre-shock average income levels. All other variables are in dollar amounts per month for all households. Panel A shows the mean, standard deviation, 10th, 25th, 50th, 75th, and 90th percentiles of each variable. In Panel B, the mean of each variable during the pre-shock period is reported separately for treated and matched households. Differences in means between treated and matched households, as well as their t-statistics, are also reported.

Panel A. Full sample statistics

				Percentiles				
Variable	N	Mean	St. dev.	10th	25th	50th	75th	90th
Number of households	5,082							
Brokerage deposits		403	2,173	0	0	100	300	720
Brokerage net flows		228	2,293	0	0	100	250	670
Brokerage withdrawals		172	1,381	0	0	0	0	0
Unemployment shocks (%)		35	54	-14	7	33	76	100
Salary income		8,403	11,607	2,326	3,964	6,412	9,804	14,603
Consumption		2,737	4,540	278	813	1,755	3,322	5,691

Panel B. Treated vs. matched households prior to shocks

Variable	Treated	Matched	Difference	t-statistic
Brokerage deposits	566	544	22	0.28
Brokerage net flows	297	355	-58	-0.76
Brokerage withdrawals	264	194	70	1.19
Salary income	7,642	7,753	-110	-0.71
Consumption	2,127	2,225	-98	-1.69

Table 2. Household Brokerage Flow Responses to Transitory Income Shocks

This table presents results from difference-in-differences regressions (equation 1) of household level brokerage flows (*Deposits, Net Flow*, or *Withdrawals*) on a dummy variable indicating whether the household experiences an income shock during our sample period (*Treat*) and dummy variables indicating three subperiods: (i) the income shock period (or pseudo-shock for the matched household) identified as the period when the treated household received unemployment insurance payments (*Shock*), (ii) the first six months following the resolution of the shock (*PostShockST*), and (iii) at least seven months after the end of the shock (*PostShockLT*). The *PostShockLT* dummy is further decomposed into <1yr and >1yr in even-numbered specifications. Brokerage flow variables are scaled by the household's average weekly salary income during the three months prior to entering our sample. The sample period is from August, 2010 to May, 2015. Household, week, and matched pair fixed effects are gradually added to the specifications. Standard errors are adjusted for clustering at the household level (*** p<0.01, ** p<0.05, * p<0.1).

	Dependent variable:									
	Deposits						Net Flow		Withdrawals	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat × Shock	-1.115***	-1.094***	-1.116***	-1.095***	-0.829***	-0.802***	-1.004***	-0.788***	0.129	-0.0499
	[0.198]	[0.213]	[0.197]	[0.212]	[0.169]	[0.181]	[0.261]	[0.278]	[0.202]	[0.215]
Treat × PostShockST	-0.928***	-0.939***	-0.929***	-0.939***	-0.929***	-0.939***	-0.613**	-0.500*	-0.327	-0.432*
	[0.175]	[0.193]	[0.175]	[0.192]	[0.175]	[0.192]	[0.271]	[0.298]	[0.219]	[0.240]
Treat × PostShockLT	-0.794***		-0.794***		-0.793***		-0.516*		-0.281	
	[0.196]		[0.196]		[0.188]		[0.275]		[0.208]	
Treat × PostShockLT(<1yr)		-0.715***		-0.715***		-0.715***		-0.288		-0.394*
		[0.202]		[0.201]		[0.202]		[0.302]		[0.234]
Treat × PostShockLT(>1yr)		-0.806***		-0.806***		-0.794***		-0.493		-0.314
		[0.227]		[0.227]		[0.216]		[0.316]		[0.238]
Shock	-0.0725	-0.0907	0.114	0.138	-0.0954	-0.0842	-0.148	-0.143	0.0975	0.107
	[0.136]	[0.146]	[0.142]	[0.155]	[0.113]	[0.122]	[0.174]	[0.190]	[0.136]	[0.150]
PostShockST	-0.395***	-0.366***	-0.0912	0.0182	-0.0417	0.0105	-0.224	-0.203	0.188	0.220
	[0.117]	[0.131]	[0.145]	[0.167]	[0.126]	[0.146]	[0.191]	[0.217]	[0.163]	[0.187]
PostShockLT	-0.721***		-0.358*		-0.146		-0.308		0.178	
	[0.130]		[0.206]		[0.152]		[0.226]		[0.188]	
PostShockLT(<1yr)		-0.705***		-0.204		-0.177		-0.353		0.178
		[0.129]		[0.200]		[0.176]		[0.258]		[0.216]
PostShockLT(>1yr)		-0.733***		-0.191		-0.0640		-0.122		0.0953
		[0.153]		[0.279]		[0.234]		[0.327]		[0.263]
Treat	0.432*	0.440*	0.432*	0.440*						
	[0.228]	[0.243]	[0.227]	[0.243]						
Obs.	661072	605025	661072	605025	661072	605025	661072	605025	661072	605025
Household FE	N	N	N	N	Y	Y	Y	Y	Y	Y
Week FE	N	N	Y	Y	Y	Y	Y	Y	Y	Y
Matched Pair FE	N	N	N	N	Y	Y	Y	Y	Y	Y
Adj. R^2	0.001	0.001	0.009	0.009	0.152	0.146	0.094	0.093	0.112	0.116

Table 3. Household Brokerage Flow Dynamics

This table reports the dynamics of household level brokerage deposits before and after temporary income shocks (or pseudo-shocks for matched households). We estimate equation 2 separately for treated and matched households (columns 1 and 2), or equation 3 on the pooled full sample (column 3). Panel A shows the dynamics for up to seven months or more after the passing of shocks, and Panel B further disaggregates the $PostShock[7m+]$ dummy into monthly dummies for up to 14 months or more. Household, matched pair (column 3), and week fixed effects are included. Standard errors are adjusted for clustering at the household level (*** p<0.01, ** p<0.05, * p<0.1).

Panel A. Baseline dynamics

	Dependent variable: Deposits		
	Treated	Matched	Pooled
	(1)	(2)	(3)
PreShock[2m]	-0.334*	-0.0922	-0.0916
	[0.188]	[0.165]	[0.163]
PreShock[1m]	-0.280	-0.107	-0.108
	[0.207]	[0.167]	[0.166]
Shock	-1.093***	-0.190	-0.162
	[0.197]	[0.156]	[0.154]
PostShock[1m]	-1.305***	-0.199	-0.144
	[0.233]	[0.201]	[0.200]
PostShock[2m]	-1.075***	-0.140	-0.0844
	[0.224]	[0.204]	[0.199]
PostShock[3m]	-1.074***	-0.0828	-0.0231
	[0.245]	[0.214]	[0.208]
PostShock[4m]	-1.030***	-0.291	-0.227
	[0.236]	[0.207]	[0.195]
PostShock[5m]	-0.881***	-0.158	-0.0940
	[0.251]	[0.214]	[0.202]
PostShock[6m]	-1.157***	-0.133	-0.0689
	[0.244]	[0.219]	[0.206]
PostShock[7m+]	-1.002***	-0.289	-0.208
	[0.249]	[0.214]	[0.185]
Treat × PreShock[2m]			-0.245
			[0.247]
Treat × PreShock[1m]			-0.175
			[0.265]
Treat × Shock			-0.969***
			[0.237]
Treat × PostShock[1m]			-1.238***
			[0.297]
Treat × PostShock[2m]			-1.071***
			[0.283]
Treat × PostShock[3m]			-1.138***
			[0.298]
Treat × PostShock[4m]			-0.897***
			[0.276]
Treat × PostShock[5m]			-0.883***
			[0.291]
Treat × PostShock[6m]			-1.186***
			[0.287]
Treat × PostShock[7m+]			-0.933***
			[0.245]
Obs.	330368	330702	661072
Household FE	Y	Y	Y
Week FE	Y	Y	Y
Matched Pair FE	N	N	Y
Adj. R^2	0.145	0.164	0.152

Table 3. Household Brokerage Flow Dynamics (continued)

Panel B. Extended dynamics

	Dependent variable: Deposits		
	Treated	Matched	Pooled
	(1)	(2)	(3)
PostShock[7m]	-0.808***	-0.503*	-0.343
	[0.299]	[0.257]	[0.225]
PostShock[8m]	-0.857***	-0.526*	-0.360
	[0.309]	[0.280]	[0.243]
PostShock[9m]	-0.965***	-0.571**	-0.405*
	[0.307]	[0.276]	[0.237]
PostShock[10m]	-0.897***	-0.149	0.0164
	[0.326]	[0.295]	[0.254]
PostShock[11m]	-0.832**	-0.340	-0.162
	[0.343]	[0.312]	[0.263]
PostShock[12m]	-0.710**	-0.350	-0.174
	[0.339]	[0.328]	[0.278]
PostShock[13m]	-0.905**	-0.326	-0.138
	[0.356]	[0.331]	[0.273]
PostShock[14m+]	-0.760*	-0.308	-0.0854
	[0.395]	[0.362]	[0.288]
Treat × PostShock[7m]			-0.595*
			[0.308]
Treat × PostShock[8m]			-0.632**
			[0.317]
Treat × PostShock[9m]			-0.693**
			[0.298]
Treat × PostShock[10m]			-1.043***
			[0.317]
Treat × PostShock[11m]			-0.810***
			[0.313]
Treat × PostShock[12m]			-0.674**
			[0.317]
Treat × PostShock[13m]			-0.915***
			[0.311]
Treat × PostShock[14m+]			-0.845***
			[0.277]
Obs.	302428	302595	605025
Household FE	Y	Y	Y
Week FE	Y	Y	Y
Matched Pair FE	N	N	Y
PreShock/Shock/PostShock dummies	Y	Y	Y
Adj. R^2	0.148	0.150	0.146

Table 4. Household Constraints: Ex-Ante Buffers and Severity of Shocks

This table presents results from estimating the baseline DID regression (equation 1) on extreme tercile subsamples of households sorted on proxies of their constraints: (i) the household's ex ante average income during the three months prior to the temporary income shock, and alternatively, (ii) the percentage decline in income during the shock (i.e., post-shock average income plus UI benefits, relative to pre-shock average income). p-values from comparing the coefficients on the interaction terms between low and high income subsamples (or large and small shock magnitude subsamples) are also reported. The sample period is from August, 2010 to May, 2015. Household, matched pair, and week fixed effects are included in all specifications. Standard errors are adjusted for clustering at the household level (*** p<0.01, ** p<0.05, * p<0.1).

	Dependent variable: Deposits			
	Pre-shock salary income		Shock magnitude	
	Low	High	Large	Small
	(1)	(2)	(3)	(4)
Treat × Shock	-1.259***	-0.569*	-2.064***	-0.740**
	[0.273]	[0.309]	[0.490]	[0.296]
p-val: Con>Uncon	0.05		0.01	
Treat × PostShockST	-1.528***	-0.699**	-1.200***	-0.591**
	[0.300]	[0.325]	[0.425]	[0.287]
p-val: Con>Uncon	0.03		0.12	
Treat × PostShockLT	-1.287***	-0.353	-1.047**	-0.539*
	[0.316]	[0.360]	[0.529]	[0.326]
p-val: Con>Uncon	0.03		0.21	
Shock	0.298	-0.356*	-0.328	0.210
	[0.184]	[0.201]	[0.307]	[0.237]
PostShockST	0.311	-0.368	-0.0152	-0.0347
	[0.224]	[0.226]	[0.297]	[0.234]
PostShockLT	0.191	-0.744**	-0.0389	0.0186
	[0.263]	[0.289]	[0.419]	[0.301]
Obs.	223974	216379	112512	113564
Household FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
Matched Pair FE	Y	Y	Y	Y
Adj. R^2	0.191	0.125	0.190	0.206

Table 5. Active vs. Passive Investments

This table presents results from estimating the baseline DID regression (equation 1) using the active or passive components of brokerage deposits as dependent variables (columns 1 and 2), or on subsamples of active or passive households (columns 3 and 4). In columns 1 and 2, brokerage deposit transactions are defined as passive if they occur in fixed intervals and in identical amounts for at least six transaction occurrences, and otherwise defined as active. Active and passive components of deposits are then aggregated at the household-week level. In columns 3 and 4, households are grouped as passive if they made at least 75% of their brokerage deposits in regular intervals and recurring amounts (i.e., up to five transaction values that recur in fixed patterns) during the 52 week period prior to the shocks, and otherwise grouped as active. p-values from comparing the coefficients on the interaction terms between the active and passive deposit specifications (or active and passive household subsamples) are also reported. The sample period is from August, 2010 to May, 2015. Household, matched pair, and week fixed effects are included in all specifications. Standard errors are adjusted for clustering at the household level (*** p<0.01, ** p<0.05, * p<0.1).

	Dependent variable: Deposits			
	Deposit component		Household type	
	Active	Passive	Active	Passive
	(1)	(2)	(3)	(4)
Treat × Shock	-0.629***	-0.151**	-1.139***	-0.531***
	[0.156]	[0.0605]	[0.294]	[0.155]
p-val: Active>Passive	0.00		0.03	
Treat × PostShockST	-0.763***	-0.0736	-1.263***	-0.594***
	[0.164]	[0.0668]	[0.313]	[0.148]
p-val: Active>Passive	0.00		0.03	
Treat × PostShockLT	-0.638***	-0.102	-0.945***	-0.659***
	[0.170]	[0.0763]	[0.327]	[0.176]
p-val: Active>Passive	0.00		0.22	
Shock	-0.388***	0.267***	-0.265	0.114
	[0.104]	[0.0460]	[0.202]	[0.0914]
PostShockST	-0.444***	0.322***	-0.177	0.0962
	[0.117]	[0.0558]	[0.224]	[0.117]
PostShockLT	-0.601***	0.412***	-0.425	0.101
	[0.136]	[0.0703]	[0.261]	[0.152]
Obs.	661072	661072	339825	321247
Household FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
Matched Pair FE	Y	Y	Y	Y
Adj. R^2	0.107	0.198	0.141	0.160

Table 6. Anchoring on Forgone Investment Returns

This table presents results from estimating the baseline DID regression (equation 1) on extreme tercile subsamples of households sorted on the cumulative return on the S&P500 index during the treated household's income shock period. Active (columns 1 and 2) and passive (columns 3 and 4) components of brokerage deposits are used as alternative dependent variables. p-values from comparing coefficients on the interaction terms between the high and low forgone return subsamples are also reported. The sample period is from August, 2010 to May, 2015. Household, matched pair, and week fixed effects are included in all specifications. Standard errors are adjusted for clustering at the household level (*** p<0.01, ** p<0.05, * p<0.1).

	Dependent variable: Deposits			
	Active deposits		Passive deposits	
	Stock market performance during shock		Stock market performance during shock	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
Treat × Shock	-0.799***	-0.754**	-0.141	-0.163*
	[0.251]	[0.301]	[0.102]	[0.0968]
p-val: High>Low	0.45		0.44	
Treat × PostShockST	-1.076***	-0.391	-0.0264	-0.13
	[0.296]	[0.264]	[0.123]	[0.109]
p-val: High>Low	0.04		0.26	
Treat × PostShockLT	-0.790***	-0.383	-0.0964	-0.257*
	[0.295]	[0.289]	[0.127]	[0.137]
p-val: High>Low	0.16		0.19	
Shock	-0.24	-0.137	0.310***	0.289***
	[0.177]	[0.197]	[0.0805]	[0.0751]
PostShockST	-0.122	-0.500***	0.347***	0.325***
	[0.236]	[0.177]	[0.0999]	[0.0852]
PostShockLT	-0.331	-0.856***	0.424***	0.569***
	[0.263]	[0.220]	[0.112]	[0.116]
Obs.	251062	204550	251062	204550
Household FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
Matched Pair FE	Y	Y	Y	Y
Adj. R^2	0.125	0.103	0.199	0.199

Table 7. Effects on Consumption and Savings

This table presents results from estimating the baseline DID regression (equation 1) using consumption and interest income – both scaled by the average income of the household over the three-month period prior to entering our sample – as dependent variables (Panel A), or on extreme tercile subsamples sorted on the magnitude of percentage decline in household consumption during the temporary income shock period (Panel B). In Panel B, p-values from comparing the coefficients on the interaction terms between the low and high consumption decline subsamples are also reported. The sample period is from August, 2010 to May, 2015. Household, matched pair, and week fixed effects are included in all specifications. Standard errors are adjusted for clustering at the household level (*** p<0.01, ** p<0.05, * p<0.1).

Panel A. Consumption and interest income as dependent variables

	Dependent variable:	
	Consumption	Interest income
	(1)	(2)
Treat × Shock	0.319	0.00376**
	[0.546]	[0.00159]
Treat × PostShockST	0.504	0.0015
	[0.620]	[0.00170]
Treat × PostShockLT	0.552	-0.000582
	[0.703]	[0.00209]
Shock	-1.169***	-0.00199*
	[0.414]	[0.00105]
PostShockST	-1.184**	-0.00167
	[0.502]	[0.00132]
PostShockLT	-1.627**	-0.000937
	[0.649]	[0.00179]
Obs.	661072	644267
Household FE	Y	Y
Week FE	Y	Y
Matched Pair FE	Y	Y
Adj. R^2	0.397	0.211

Table 7. Effects on Consumption and Savings (continued)

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Panel B. Brokerage deposit responses and consumption changes

	Dependent variable: Deposits	
	ΔConsumption during shock	
	Low	High
	(1)	(2)
Treat × Shock	-0.976***	-0.786**
	[0.310]	[0.315]
p-val: High>Low	0.33	
Treat × PostShockST	-1.071***	-0.943***
	[0.318]	[0.318]
p-val: High>Low	0.39	
Treat × PostShockLT	-0.881***	-0.933***
	[0.338]	[0.338]
p-val: High>Low	0.46	
Shock	-0.254	-0.178
	[0.199]	[0.204]
PostShockST	-0.0528	-0.0823
	[0.223]	[0.228]
PostShockLT	-0.124	-0.273
	[0.275]	[0.263]
Obs.	212364	230701
Household FE	Y	Y
Week FE	Y	Y
Matched Pair FE	Y	Y
Adj. R^2	0.148	0.157